Referee Report on

Estimating tubulent energy flux vertical profiles fro uncrewed aircraft system measurements: Exemplary results for the MOSAiC campaign

The authors present an overview of their approach to derive turbulent statistics, particularly dissipation rate, ε, normal Reynolds stresses (variances of velocity fluctuation), eddy diffusivities, and heat and moisture fluxes using uncrewed aerial system (UAS) measurements. I found the paper to be well written and found that it does a good job of reviewing most of their calculation approaches and provides a careful and honest examination of the authors' results. This information is potentially useful for other researchers interested in extracting similar statistics or for researchers who are interested in the details of the calculations when examining data produced by the aircraft. I therefore feel that the article is suitable for publication in Atmospheric Measurement Techniques.

One major concern I had was in the description and usage of the turbulent kinetic dissipation rate, ε. Although by definition this is a scalar quantity (describing the rate of viscous dissipation of the scalar quantity turbulent kinetic energy), the authors repeatedly refer to it as having different components (ε_U and ε_w) as well as having anisotropy between these components. I believe that this may stem from a misunderstanding of equation 8 in the manuscript, whereby they describe the wavenumber as a scalar property, whereas it is actually a vector quantity. Given that they are applying Taylor's frozen flow hypothesis, it appears that Equation 8 is actually describing the longitudinal wavenumber spectrum, in which case $E(k)$ describes the energy content of the velocity component parallel to the wavenumber, k. This then means that the Kolmogorov constant α for the w component of velocity should actually be closer to 0.65 (from Pope (2000), page 232) leading to a difference in ε estimates of about 30%, when using the frequency spectrum to estimate it, which could explain the difference the authors are seeing in their values of ε_w an ε_u. Alternativley the different noise floor for the U and w components of velocity from the sonic on the BELUGA could also impact their estimates from the second order structure function, if not properly accounted for. Either way, I found the description of ε as having different components to be confusing.

I am also concerned with their parameterization of anisotropy, as anisotropy is a relic of the boundary conditions at which the turbulence is produced it is unlikely to be readily parameterized, particularly near a surface. Furthermore, limiting to relatively small scales the calculation of σ_u^2 and σ_v^2 could also be impacting the value of their anisotropy estimates due to the difference in the expected inertial subrange scaling for longitudinal vs lateral wavenumber spectra mentioned above.

Other, minor, comments that I also feel should be addressed before publication are as follows:

1. [Line 100] It would be valuable for the authors to provide the radius of their spiral ascent/descent, as this impacts the validity of how they apply Taylor's hypothesis in Eq. 8. (Tighter spirals are not likely to provide effective approximations of longitudinal spectra).

2. [Line 117] How is the 2 s used to estimate the spectrum determined? For a 15 m s^{-1} airspeed, this would only correspond to eddies of wavelengths shorter than 30 m. One would expect integral scales to roughly increase with altitude, such that eddies on the order of 100 m or larger could be present for the boundary layers shown in Figs. 9,10 11 and the full energy content not captured in these quantities.

3. [Line 118] I am curious about the frequency response for the pitot probe. Typically the pitot tube would experience some attenuation at high frequency due to viscous damping and resonance at certain frequencies due to the transducer cavity. In Hamilton et al (2022) only the hot-wire probe is cited as having the O(1kHz) frequency response so the use of the pitot probe for these calculations requires some justification.

4. [Line 143] "and temporary 23 m made " should perhaps be "and temporarily 23 m were made".

5. [Line 150] There should be a space between 23 and m.

6. [Line 178] σ_w should be specifically defined to refer to the standard deviation of the vertical component of velocity. Similarly, the quantity ε_w should also be more specifically defined since ε is later defined as the dissipation rate.

7. [Equation 9] What is the quantity $\overline{U}_\tau$ and how does it relate to $\overline{U}$ used in equation 8?

8. [Line 219] How does the local dissipation rate ε_τ differ from ε?
9. [Line 245] Referring to Fig.2, it would appear that the frequencies above 40 Hz are elevated above the -5/3 slope. How is this impacting the dissipation rate calculation, given that it is calculated using the full 2-400 Hz spectrum?
10. [Line 249] Given that Doddi (2021) is a thesis, perhaps it would be best to have some of the details of the spectral analysis included in this paper?
11. [Line 295] As I understand the procedure being described, the authors are anchoring the fit to the lowest frequency point in the spectrum and using that as an initial reference for discarding points whose least-squares value is too high. However, is this justified given that the lowest wavenumbers are likely to be the least accurate (i.e. it represents the amplitude of only a single wave)?
12. [Figure 5] Assuming a wind speed of 5 m s^{-1} and UAS airspeed of 15 m s^{-1}, it might be worthwhile noting that the comparison here shows energy content in scales on the order of 30 m for the UAS, and between 10 and 150 m for the BELUGA. I would argue that this explains the trends observed between the different high-pass filter window lengths, but the best comparison to the UAS would be expected from the the 5 s window (depending on horizontal wind speed).
13. [Line 351] It is perhaps not surprising that that behavior of the turbulent Prandtl number is less clear for stable flows, given that its definition presumes the existence of turbulence (implicit in the eddy viscosity/eddy diffusivity definition).
14. [Line 378] "It remains open that the DH2 provides the horizontal component," horizontal component of which quantity? This sentence is unclear.
15. [Line 384] The statement that strongly turbulent flows are more isotropic than less turbulent flows is only appropriate for high wavenumbers. At small wavenumbers, strongly turbulent flows can be very anisotropic (e.g. in the neutral turbulent boundary layer).
16. [Line 406] The authors refer to A, but previously had defined only A_σ and A_ε.
17. [Line 429] q was not previously defined.
18. [Figure 9] Which value of σ^2 is being plotted, previously had defined σ_U^2 and σ_w^2. In the text (e.g. Line 503) it is referred to as σ_U^2
19. [Line 443] What is meant by this statement? Equations 8 and 9 are only valid in the inertial subrange.
20. [Line 506] The acronym LLJ was not defined.